The proper reconstruction of exchange economics

Balzer, Wolfgang

W. BALZER[1]

THE PROPER RECONSTRUCTION OF EXCHANGE ECONOMICS

Recently, F. Haslinger (in [Haslinger, 1983], in the following to be quoted in the form [H, p . . .]) has heavily but constructively criticized my reconstruction of pure exchange economics (PEE in [Balzer, 1982], in the following to be quoted in the form [B, p . . .] or [B]) from the point of view of the working economist. I want to deal here with three of his objections because these – although advanced and discussed in connection with a special theory, PEE – are of relevance to three corresponding problems of meta-science in general. The problems are the following: (1) choice of adequate primitives, (2) choice of adequate axioms, and (3) inadequacy of reconstructions because of prejudices caused by meta-theory. Haslinger's criticism amounts to stating that my reconstruction fails in all three respects: I have chosen the wrong primitives and axioms, and my reconstruction suffers from an inadequacy due to my being misled by some wrong meta-theoretical intuition. I will try to defend myself on the first two items, and agree with Haslinger only on the third one – but even there not without stressing the meta-scientific revenue of my mistake. I believe that both sides, working scientists as well as philosophers of science, do profit from such kinds of controversies, and I can only deplore their being so rare.

I. CHOICE OF ADEQUATE PRIMITIVES

On my account in [B], PEE is formulated with the following primitives: individuals ($\mathcal{F}$), kinds of commodities (G), total quantities available ($\bar{q}$), initial outfit of individuals (q^0), prices (p), utilities (U) and equilibrium distributions (E).[2] Haslinger's first objection is that I do not use the concept of a demand function as a primitive. Demand functions are essential to exchange theories, therefore my choice of primitives is inadequate. "Balzer does not make explicit use of the concept of a demand function, which is a crucial one in economic theory as is clear from a careful study of the texts quoted by him on p. 24" [H, p. 119].

Erkenntnis **23** (1985) 185–200. 0165–0106/85.10

"Balzer completely neglects this concept, which is, of course, one of the most important ones in economic theory" [H, p. 117].

I think one can separate further apparent consequences for the notion of equilibrium (to which I will come in section II) from the mere neglect of the concept as expressed in the above quotation. But then what is at stake is this. I have chosen a version of PEE working without demand function while Haslinger and perhaps some other economists would prefer to have the demand function as a primitive. For further clarification let us recall Haslinger's definition. With some minor technical changes, and with $EB_i(p, q_i)$ as an abbreviation for $\{q'_i/p \cdot q'_i \leqq p \cdot q_i\}$, it can be formulated in the following way.

D1. x is a *potential model* of PEE* ($x \in M^*_p$) iff

$$x = \langle F, G, (q^0_i)_{i\in F}, (U_i)_{i\in F}, (q^a_i)_{i\in F}, p, (d_i)_{i\in F}\rangle$$

and

(1) F is a finite, non-empty set[3] and $G = \{1, \ldots, m\} \subseteq \mathbb{N}$;
(2) for all $i \in F$: $q^0_i \in \mathbf{R}^m_+$, $q^a_i \in \mathbf{R}^m_+$ and $U_i : \mathbf{R}^m \to \mathbf{R}$ is smooth, monotonic, and strictly quasi-concave;
(3) $p \in \mathbf{R}^m_{++}$;
(4) for all $i \in F$ and all $p' \in \mathbf{R}^m_{++}$:
(4.1) $d_i : \mathbf{R}^m_{++} \times \mathbf{R}^m_+ \to \mathbf{R}^m_+$;
(4.2) $d_i(p', q^0_i) \in EB_i(p', q^0_i)$ and for all $q^*_i \in EB_i(p', q^0_i)$: $U_i(q^*_i) \leqq U_i(d_i(p', q^0_i))$;
(4.3) $d_i(p, q^0_i) = q^a_i$.

Here, F is the set of individuals, G the set of kinds of commodities. Each q^0_i and q^a_i is a commodity vector ("outfit" and "actual demand"), U_i is the utility function of individual i, and d_i its demand function. p is the price vector. $EB_i(p', q_i)$ represents i's budget constraint relative to outfit q, and at prices p'. (D1-4.2) says that i maximizes his utility under the restriction of his budget. In (4.3) I have idealized Haslinger's approximative equality "$\approx$" to a strict one. We have the following theorem.

T1. (a) In $x \in M^*_p$, the function $d_i(\cdot, q^0_i)$: $\mathbb{R}^m_{++} \to \mathbb{R}^m_+$, defined by $d_i(\cdot, q^0_i)(z) = d_i(z, q^0_i)$ is uniquely determined for all $i \in F$.

(b) If in (D1) requirement (4.2) is replaced by

(4.2)* for all $q_i : d_i(p', q_i) \in \mathrm{EB}_i(p', q_i)$ and for all $q_i^* \in \mathrm{EB}_i(p', q_i)$: $\mathrm{U}_i(q_i^*) \leqq \mathrm{U}_i(d_i(p', q_i))$,
then for all $i \in \mathrm{F}$: d_i is uniquely determined in M_p^*.

This follows, for instance, from (T3) of [B] as a special case because any monotonic and strictly quasi-concave U_i which is twice differentiable will satisfy the assumptions used there. But uniqueness in each structure means definability[4] and thus eliminability. That is, the "economically relevant part" of the demand function (T1-a) or even the "complete" function (T1-b) can be explicitly defined and in this sense is redundant in M_p^*. Since the assumptions of (T1-b) are only slightly stronger than Haslinger's as far as the form of the utility functions is concerned, I see no reason why (D1-4.2) should not be replaced by these assumptions in general. Similarly, if we start from models of PEE (elements of the set M) which do not contain the demand function we there can choose an appropriate specialization $\mathrm{M}^c \subseteq \mathrm{M}$ (see section IV), and then in structures of M^c introduce the d_i by definition. Again, this is justified by (T3) of [B].[5]

So, despite of the explicit neglect of demand functions in my version they nevertheless are implicitly (potentially) present. Their neglect therefore, or so it seems, does not point to any inadequacy of my choice of primitives.

The general lesson to be drawn from this example is, of course, that the mere list of primitives is no good standard of comparison. In order to evaluate (or even to prove) that some reconstruction fails to exhibit a certain concept we have to take into account not only the primitives but also all concepts which are definable in the theory under consideration.

II. CHOICE OF ADEQUATE AXIOMS

Here, my treatment of "cleared markets" is under attack:

(c) $\sum_{i \in \mathcal{F}} [q^0(i, j) - q(i, j)] = 0$ for all $j \in \mathrm{G}$, where $q \in \mathrm{E}$ is some "equilibrium" distribution.

I have two remarks on this point, one more formal and the other more "inhaltlich" in nature. In my reconstruction I refer to the distinction between the "basic core" of a theory and its "specializations", the former may be said to be characterized by "basic axioms" or "basic laws", the latter by "special axioms" or "special laws". In the history of theories one frequently meets laws (axioms) to which during the development of the theory there are added other laws which logically represent special cases of the former. (Newton's second law and Maxwell's equations are typical basic laws in physics). Basic laws provide a kind of frame which predetermines the way in which specializations possibly can be chosen. They remain unchanged over a long period, but the reason of their not being changed is that they are empirically vacuous. Since no clash with reality is possible there also is no need of emendation or reformulation. Special axioms, on the other hand, are empirically non-trivial, and they allow for refutations and predictions (Newton's law of gravitation and Hooke's law are examples to the point.[6]

Typically, basic laws are "cluster-laws", i.e., laws in which all or most of the primitives of a theory occur together. It is an empirical hypothesis of meta-science that basic laws are always cluster-laws (while special laws can be but need not). In PEE the law of maximization of utilities under budget constraints clearly is *the* cluster law. This claim is meant in a purely formal way, and it can be checked simply by counting the number of primitives occurring in the different axioms. The "clearing of markets" requirement, in contrast, is not very efficient in clustering. It relates only two concepts, namely "equilibrium" E ("q" varies in the set E) and "initial endowment" (if we ignore the role of $\mathcal{F}$ as a mere index set).

Another important feature of this distinction is that special laws are not valid in *all* intended applications of the theory; they are valid only in a subclass of intended applications which is relatively small in comparison to the class of intended applications of the whole theory but still big enough to justify a systematic investigation. Hooke's law, for instance, is a special law of mechanics which is not valid in all mechanical systems; it only holds for particles being suspended from springs and similar systems. The clearance of markets requirement, too, seems to be satisfied only in a subclass of all intended applications of exchange theory. It requires low cost of information and transaction, and there are clear situations which – on the premiss that exchange theory has intended applications at all – should be regarded as intended applications of PEE but not of the special requirement under discussion. Also, the current widespread studies of disequilibrium systems in economics points to this fact. On the Saturday morning food market of a town there may be excess demand for one or even two Saturdays *without* prices going up. It looks like "escapism" if such examples are "argued away" by pointing out that PEE of course is only an idealized theory. If there are any intended applications at all they will be of such imperfect nature.

With regard to these considerations it seems not inadequate to treat the "clearance of markets" requirement as a special law – as I did in [B]. The axiom is empirically non-trivial, it formally narrows down the class of models. It does not have the form of a cluster-law, and it also is not valid in all intended applications of PEE. Note that treating an axiom as a special law does not mean to exclude it from the theory. If a distinction between basic and special laws is drawn "the theory" has to be reconstructed as a *theory-net*[7] in the formal sense, and all questions concerning "the theory" have to be evaluated with respect to such a net.

A second remark refers to the concept of equilibrium. Haslinger stresses that without clearance of all markets there can be no equilibrium. "For an allocation to be a Walrasian equilibrium, economists require additionally the simultaneous clearance of all markets" [H, p. 119], "Equilibrium theorists would strongly claim that as long as there are markets which are not cleared, the economy cannot be in a state of equilibrium. In particular, if there is positive excess demand (where total demand exceeds total supply) on a market for a certain commodity, there is still a tendency for a price increase. This leads in turn to a reduction of its demand and thereby to a decrease of excess demand for that commodity" [H, p. 120]. From this one might conclude that my not treating the axiom under consideration as basic leads to a concept of equilibrium – as characterized by the basic axioms – which is economically inadequate. Neglecting what I said before about theory-nets the situation seems to be this.

Economists – as all working scientists – have a realist mode of talking. The word "equilibrium" refers to equilibrium states "in reality". The real equilibrium states are primary, the corresponding concept(s) of equilibrium used in different theories about the economic world has (have) to be checked against those real states. There is common agreement that an economic system is in a state of equilibrium *only if* all agents maximize their utilities and all markets are cleared. A further necessary condition – which usually is not mentioned – is that during a short period prices, utilities and total quantities available have to be *constant*. Note that I am talking about real equilibrium as prior to any economic theory here, and in reality time always is involved. I doubt whether a system in which prices, utilities and demands *are changing at any moment* but which nevertheless happens to satisfy the requirements of maximization of utilities and clearance of markets at any moment would be said to be in *equilibrium*. This is no mere speculation, such systems can be constructed mathematically, and therefore are possible. Let me denote by "equilibrium$_{\text{real}}$" the concept economists have in mind.

On the other hand, if some economic theory like PEE has been introduced it may occur that among the primitives of the theory we find a term which is called "set of equilibrium states". This term then is characterized or "implicitly defined" by the axioms of the given theory – and by nothing else. Let me denote by "equilibrium$_{\text{T}}$" the term as implicitly defined by economic theory T, and in particular by "equilibrium$_{\text{PEE}}$" the term as characterized by PEE.

Then Haslinger's objection as exemplified by the above quotations as well as my remarks in [B] on the notion of equilibrium both start from the premiss that equilibrium$_{\text{real}}$ = equilibrium$_{\text{PEE}}$. That is, we both thought that the concept of equilibrium as characterized by PEE was or should be the same as *the* real concept of equilibrium in economics. It now seems to me that this premiss is wrong. I doubt whether a statical (or quasi statical) theory like PEE possibly could characterize the concept of equilibrium$_{\text{real}}$. If my above remark that constancy of prices and other functions over time is necessary for equilibrium$_{\text{real}}$ is accepted then PEE clearly is unable to characterize equilibrium$_{\text{real}}$, simply because PEE does not refer to time. If, on the other hand, economists would agree on maximization of utilities and clearance of markets to be necessary *and* sufficient for the characterization of equilibrium$_{\text{real}}$ then, indeed, I would feel forced to treat the second axiom as basic, too. I am not convinced, however, that there is such agreement.

The general point to be made here is that the decision of whether some axiom belongs to a theory or not need not be a question of whether it is "true" in the models or applications of that theory. Rather the decision may be influenced by conceptual questions of identifying this theory by means of "inner" criteria (like "intended applications") or of "outer" criteria (like its relations to other existing theories).

III. INADEQUACY OF RECONSTRUCTIONS BECAUSE OF PREJUDICES CAUSED BY META-THEORY

More specifically, the discussion here is about theoretical terms. In my reconstruction I used the structuralist concept of the empirical theory, and I tried to subsume the case in question, PEE, under this general concept. Now according to the structuralist view one crucial feature of an empirical theory is the distinction between theoretical and non-theoretical terms which is drawn by means of the following intuitive or pragmatic criterion. Term $\bar{t}$ of theory T is T-*theoretical* iff in every process of measurement for $\bar{t}$, T is presupposed as valid. Here, a process of measurement for $\bar{t}$ is any concrete system in which, or by means of which, some value of the (concrete) function or predicate which "represents" $\bar{t}$ in that system is uniquely determined (and thus can be found or measured by collecting or presupposing information about the other terms represented in the system). And to presuppose T as valid in such a process of measurement means that the structure consisting of the objects, functions and predicates (which represent T's terms) realized in the concrete system is a model of T.

Applying this criterion which was first proposed by Sneed in [Sneed, 1971], I arrived at the conclusion that in PEE "utility" (i.e., the term which in models of PEE is represented by a utility function) is PEE-theoretical. Roughly, the idea was that the usual methods of determination (or measurement) for preferences (and thus for an ordering among utility-values) already contain (and in this sense presuppose) the requirement that people in the course of actions (like the measurements in question) try to maximize their utilities. For without this presupposition the alternative actually chosen might as well represent the worst one on the individual's scale, e.g., because of "misunderstanding" of the instructions of the experimentator or the "rules" of the "economic game".

I believe that this observation is correct but on the other hand I now agree with Haslinger (and "practically all" economists) that, in fact, "utility" is *not* PEE-theoretical (contrary to my finding in [B] and in line with Haslinger's criticism). How is that possible? The answer is easy enough although perhaps a bit surprising: I have changed my meta-theory. This sounds a bit unserious, and like the introduction of a Popperian *ad hoc* hypothesis. But it is not. Actually, the meta-theoretical change took place independently of considerations of the example of PEE. U. Gaehde in [Gaehde, 1983] proposed a new and improved criterion of theoreticity, and I simply was pleased to see that a modified version of this new criterion in the case of PEE led to a distinction different from the one drawn on the basis of Sneed's original criterion: according to the new criterion "utility" turned out as PEE-non-theoretical. I will state this result now in some detail, and afterwards try to reevaluate the original criterion of theoreticity in the light of the new one.

The basic intuition underlying the new criterion is this. Term $\bar{t}$ of theory T is T-theoretical if $\bar{t}$ can be determined or measured by means of T, and $\bar{t}$ is T-non-theoretical if it is *not possible* to determine $\bar{t}$ by means of T. Possibilities of determination are formally described by certain models of T in which the function or relation representing $\bar{t}$ is uniquely determined (eventually up to transformations of scale).

More precisely, let us assume that the models of T have the form $\langle D_1, \ldots, D_k; R_1, \ldots, R_m\rangle$ where $D_1, \ldots, D_k$ are sets (of "objects") and $R_1, \ldots, R_m$ are relations or functions "over" $D_1, \ldots, D_k$. Let M denote the class of all models of T. Then the i-th *term* of T (for $i = 1, \ldots, m$) is defined by

$$\bar{R}_i = \{R_i / \exists D_i \ldots D_k, R_1 \ldots R_{i-1}, R_{i+1}, \ldots, R_m(\langle D_1, \ldots, D_k; R_1, \ldots R_m\rangle \in M)\}.$$

We say that $\bar{R}_i$ is *real-valued* if for all $R \in \bar{R}_i$, R is a function taking its values in the set of real numbers. Two functions or relations $R, R' \in \bar{R}_i$ are called *scale-equivalent* if either $\bar{R}_i$ is not real-valued and $R = R'$ or if $\bar{R}_i$ is real-valued and R' can be obtained from R by a transformation of the form $x \to \alpha \cdot x$ with $\alpha \in \mathbb{R}_{++}$. Equivalence of scale is denoted by $R \triangleq R'$, i.e., for $R, R' \in \bar{R}_i$: $R \triangleq R'$ iff ($\bar{R}_i$ is not real-valued and $R = R'$) or ($\bar{R}_i$ is real-valued, $\mathrm{Dom}(R) = \mathrm{Dom}(R')$ and there is $\alpha \in \mathbb{R}_{++}$ such that for all $b \in \mathrm{Dom}(R)$: $R'(b) = \alpha \cdot R(b)$).[8] Finally, let us introduce the notation $x_{-i}[R]$ (where $x \in M$, $i \leqq m$ and $R \in \bar{R}_i$) for the result of substituting R for the $(k+i)$-th component of x. Thus if $x = \langle D_1, \ldots, D_k; R_1, \ldots, R_m\rangle$ then $x_{-i}[R] = \langle D_1, \ldots, D_k; R_1, \ldots, R_{i-1}, R, R_{i+1}, \ldots, R_m\rangle$.

Now according to the new criterion term $\bar{R}_i$ is T-*theoretical* if $\bar{R}_i$ is weakly and invariantly definable in T. This indicates a twofold modification of the requirement of $\bar{R}_i$ being definable in T. Recall that $\bar{R}_i$ is definable in T iff[9] in each model of T, R_i is uniquely determined:

$$\forall x \forall \mathrm{R}, \mathrm{R}' \in \bar{\mathrm{R}}_i (x_{-i}[\mathrm{R}] \in \mathrm{M} \wedge x_{-i}[\mathrm{R}'] \in \mathrm{M} \rightarrow \mathrm{R} = \mathrm{R}').$$

This requirement is modified in two respects. First, definability is weakened to "weak definability". We call $\bar{\mathrm{R}}_i$ *weakly definable* in T iff there is some "subtheory" T′ of T, given by a subset B of M so that $\bar{\mathrm{R}}_i$ is definable in T′. This requirement on its own would be trivial, of course, for each term is weakly definable in any T. But in a second step we add a condition of invariance restricting the choice of B (i.e., of the subtheory employed for the definition). We allow for subtheories T′ (subclasses $\mathrm{B} \subseteq \mathrm{M}$) only if they are invariant under "replacement of R_i as governed by M". We say that a *replacement* of R_i in x by R_i' is *governed by* M if the result, i.e., $x_{-i}[\mathrm{R}_i']$, again belongs to M. And we say that $\mathrm{B} \subseteq \mathrm{M}$ is *invariant under replacement governed by* M (or simply that B is M-*i-invariant*) iff for all $x \in \mathrm{B}$ and all replacements of R_i in x by R_i' governed by M the result $x_{-i}[\mathrm{R}_i']$ again is in B:

$$\forall x \forall \mathrm{R} \in \bar{\mathrm{R}}_i (x \in \mathrm{B} \wedge x_{-i}[\mathrm{R}] \in \mathrm{M} \rightarrow x_{-i}[\mathrm{R}] \in \mathrm{B}).$$

D2. Let T be a theory with class M of models of the form $x = \langle \mathrm{D}_1, \ldots, \mathrm{D}_k; \mathrm{R}_1, \ldots, \mathrm{R}_m \rangle$ and let $i \leqq m$. The i-th term of T, $\bar{\mathrm{R}}_i$, is called T-*theoretical* iff there is some $\mathrm{B} \subseteq \mathrm{M}$ such that $\mathrm{B} \neq \emptyset$ and

(1) $\forall x \forall \mathrm{R}, \mathrm{R}' \in \bar{\mathrm{R}}_i (x_{-i}[\mathrm{R}] \in \mathrm{B} \wedge x_{-i}[\mathrm{R}'] \in \mathrm{B} \rightarrow \mathrm{R} \triangleq \mathrm{R}')$,

(2) $\forall x \forall \mathrm{R} \in \bar{\mathrm{R}}_i (x \in \mathrm{B} \wedge x_{-i}[\mathrm{R}] \in \mathrm{M} \rightarrow x_{-i}[\mathrm{R}] \in \mathrm{B})$.

Note that in (D2-1) we use "$\triangleq$" instead of "$=$". In most cases the determination of a function by means of theoretical laws is possible only up to transformations of scale because usually theories are invariant under "choice of units" and therefore contain invariances of scale.[10]

The intuition behind this criterion is the following. T-theoretical terms are those which are first introduced together with T and thus get their meaning only in the context of T, and by means of T. And "to get meaning by means of T" here is expressed by the *possibility* of determining the term under special, favourable conditions (weak definability) without violating the general invariances of T (D2-2). This intuition is in line with Putnam's statement that a theoretical term "comes from" some theory, as well as with the general beliefs in "definability" of theoretical terms hold by logical empiricists and subsequent research traditions.

On the basis of this new criterion it is possible to *prove* that in PEE the "right" terms are PEE-theoretical. From (D0) and (D2) we obtain the following theorem.

T2. If the following requirements are added to the axioms of PEE: (A) $m \geqq 2$, (B) not for all arguments, q^0 is zero, then $\bar{q}^0$ and $\bar{\mathrm{U}}$ are PEE-non-theoretical while $\bar{p}$ and $\bar{\mathrm{E}}$ are PEE-theoretical.[11]

The two additional requirements (A) and (B) seem to be essential to PEE. I agree that neglecting them in [B] represents an inadequacy. On the other hand this again is a nice example for the interplay between specific reconstruction and meta-theory. The theorems proved in [B] go through in the degenerate cases excluded by (A) and (B) above. But without (A) and (B) the distinction between theoretical and non-theoretical terms does not come out in the right way. Besides, (A) and (B) are intuitively necessary because if $m = 1$ or q^0 is identical zero no exchange is possible.

A final question to be considered here is the connection of (D2) to Sneed's original criterion outlined earlier. As I see it now, after some applications and discussions of the new criterion[12], the situation seems to be this. Sneed's original version expresses a basic intuition about the identification of a concept (like "mass", "utility", "price") as *the* concept given by a certain theory. If we do not presuppose the theory during each process of determination of the concept there is no connection between the value determined by such a process and the value of the corresponding concept as *determined by* the theory. Thus without Sneed's presuppositions we cannot say that by means of a certain process we have determined *the* concept... of theory T.[13] But shouldn't this condition of identification be satisfied for *all* of T's terms (or concepts), that is, for the T-non-theoretical ones as well? And if scientists in some cases, namely in cases of non-theoretical terms, do *not* presuppose the theory during the corresponding determinations (as is claimed in the structuralist meta-theory), why so? Here the new criterion gives a clear answer: because presupposing the theory does not yield any means of determination for the terms in question. This follows directly from (D2). If a term is T-non-theoretical there is no possibility for its determination (D2-1) with corresponding B which does not violate the basic invariances of the theory (D2-2). In this sense the new criterion represents a refinement of the original one. It explains some phenomenon which in connection with the old criterion was just stated. The intuition underlying the old criterion is not affected by the new one. It only turns out to be less an intuition about theoreticity and more an intuition about the identification of concepts and about the mechanism of meaning.

In the case at hand my argument in [B] about presupposing the principle of maximization during the determination of utilities remains sound but nevertheless utilities turn out to be PEE-non-theoretical. In other words: we do not properly understand the meaning of "utility" unless we think of maximization of utility, but PEE is no theory that would yield any possibilities for determining utilities or for giving meaning to "maximization of utility".

This discussion nicely illustrates the following general point. Theories on the meta-level (if regarded as empirical theories) function just in the same way "ordinary" theories do. They draw their generalizations (here: distinction between theoretical and non-theoretical terms) on the basis of few concrete examples, they can be "corrected" on the basis of new examples, and they also provide new ways of looking at old examples. The study of examples from physics had led to a new and more precise distinction between theoretical and non-theoretical terms, i.e., to a correction of the meta-theory. The new meta-theory applied to the example of PEE led to a picture different from that drawn in [B], and also one which better seems to fit with the "phenomena" (i.e., what scientists do and say).

IV. CONCLUSION

Having reviewed three questions of detail I finally want to come to the more general question of how to identify PEE, how to determine its place in the net of economic theories. I have added the adjective "pure" in order to indicate that production does not matter. Unfortunately "pure" may also be understood as the opposite of "applied", so that "without production" would be more appropriate. In the realm of exchange theories without production there is essentially the distinction between statical and "dynamical" theories. "Statical" is meant here to include "quasistatical" theories like PEE, and "dynamical" is meant as "non-statical". Under the heading of "dynamics" several further distinctions are possible which are of no concern to the present discussion. What Haslinger reconstructs in his proper models [H, p. 126] may be regarded as a particular "dynamical" theory of exchange. But any kind of "dynamical" theory will rest on some underlying static theory[14] – or at least will contain such a theory as a "part". On Haslinger's account this theory (which is just my PEE) is embodied in his potential models [H, p. 126]. I see no objection against such a treatment but I want to stress that the potential models obtained in this way have a very rich logical structure (they satisfy some real cluster law) and thus deserve to be conceived as the models of a theory on their own. How to call that theory may be a matter of controversy, perhaps "Static Exchange Economics Without Production" would be more appropriate than PEE.

But if this trivial terminological question is neglected, and if we look at Haslinger's and my definitions of the models it becomes clear that we are talking about two different theories. He talks about some not yet fully developed "dynamical" theory while I talk about statics. This can be made more explicit by formally considering the relation between my class of models M and Haslinger's class of potential models M_p^*. As it turns out, a specialization M^c of M is equivalent with M_p^* in the following sense.[15]

D3. (a) $x \in N$ iff $x = \langle F, G, (q_i^0)_{i\in F}, (U_i)_{i\in F}, (q_i^a)_{i\in F}, p, (d_i)_{i\in F}\rangle$ and
(1) requirements (1)–(3) of (D1) are satisfied
(4) for all $i \in F$: $d_i : \mathbb{R}^m_{++} \times \mathbb{R}^m_{+} \to \mathbb{R}^m_{+}$
(b) Let M^c be defined as follows:
$x \in M^c$ iff
(1) $x \in M$ $(x = \langle \mathcal{F}, G, q^0 p, U, E\rangle)$
(2) for all $i \in \mathcal{F}$: U_i is monotonic and strictly quasiconcave
(3) for all $p' : G \to \mathbb{R}_{++}$ there is $q : \mathcal{F} \times G \to \mathbb{R}_+$ such that
(3.1) for all $i \in \mathcal{F}$: $\sum_{j\leqq m} p'(j)[q(i, j) - q^0(i, j)] \leqq 0$
(3.2) for all q', if for all $i \in \mathcal{F}$: $\sum_{j\leqq m} p'(j)[q'(i, j) - q^0(i, j)] \leqq 0$ then for all $i \in \mathcal{F}$: $U(i, q'(i, 1), \ldots, q'(i, m)) \leqq U(i, q(i, 1), \ldots, q(i, m))$
(c) Let $\rho \subseteq M_p \times N$ be defined by
$\langle \mathcal{F}, G, q^0, P, U, E\rangle \rho \langle F, G', (q_i^0)_{i\in F}, (U_i)_{i\in F}, (q_i^a)_{i\in F}, p', (d_i)_{i\in F}\rangle$ iff
(1) $\mathcal{F} = F$, $G = G'$ and $p = p'$
(2) for all $i \in F$, $g \in G$ and $\alpha_1, \ldots, \alpha_m \in \mathbb{R}$: $q^0(i, g) = q_i^0(g)$, and $U(i, \alpha_1, \ldots, \alpha_m) = U_i(\alpha_1, \ldots, \alpha_m)$
(3) $E = \{q^a\}$ where $q^a = \{\langle i, g, q_i^a(g)\rangle / i \in F \wedge g \in G\}$ and $q_i^a(g)$ denotes the g-th component of q_i^a
(4) there is $d^* : F \to \mathrm{Pot}(\mathbb{R}^m_{++} \times \mathbb{R}^m_{+} \times \mathbb{R}^m_{+})$ such that

(4.1) for all $i \in F: d^*(i): R^m_{++} \times R^m_+ \to R^m_+$ and for all $p' \in R^m_{++}$
(i) $p' \cdot d^*(i)(p', q^0_i) \leqq p' \cdot q^0_i$
(ii) for all $q'_i \in R^m_+$: if $p' \cdot q'_i \leqq p' \cdot q^0_i$ then $U(i, q'_i) \leqq U(i, d^*(i)(p', q^0_i))$
(4.2) for all $i \in F$, $p' \in R^m_{++}$ and $q_i \in R^m_+: d^*(i)(p', q_i) = d_i(p', q_i)$.

T3. Suppose in the definition of $B_x(p)$ in (D0) "$=$" is replaced by "$\leqq$". Then
(a) for all x, y: if $x\rho y$ then ($x \in M^c$ iff $y \in M^*_p$).
(b) $\check{\rho}: M^*_p \to M^c$ is bijective.

Proof: We write $U(i, q)$ for $U(i, q(i, 1), .., q(i, m))$.

(a) "$\Rightarrow$". Let $x \in M^c$ and $x\rho y$. By definition of M^c, U^x_i is monotonic and strictly quasi-concave which, by (D3-c-2) transfers to U_i ($i \in F$). Let $p': G \to R_{++}$ be given. By the definition of M^c there is $q: \mathcal{F} \times G \to R_+$ such that (D3-b-3.1) and (2) are satisfied. That is, $q \in B_x(p')$, and if we write $q = (q_i)_{i \in \mathcal{F}}$ this implies (1) $p' \cdot q_i \leqq p' \cdot q^0_i$ for all $i \in \mathcal{F}$. On the other hand, since U_i is strictly quasi-concave and monotonic, $d^*(i)(p', q^0_i)$ is uniquely determined by conditions (D3-c-4.1)-(i) and (ii), which are identical with (D3-b-3.1) and (2). It follows that (2) $d^*(i)(p', q^0_i) = q_i$ for all $i \in \mathcal{F}$. From (1), (2) and (D3-c-4.2) we obtain $p' \cdot d_i(p', q^0_i) \leqq p' \cdot q^0_i$ which proves the first part of (D1-4.2). Now let $p' \cdot q'_i \leqq p' \cdot q^0_i$. By (D3-b-3.2) this yields $U(i, q') \leqq U(i, q)$ and from (D3-c.2), (D3-c-4.2) and (2) we obtain $U_i(q'_i) = U(i, q'_i) \leqq U(i, q_i) = U(i, d_i(p', q^0_i))$ which proves the second part of (D1-4.2). By (D3-c-3), $q^a \in E$, so by (D0-2): (3) $q^a \in B_x(p)$ and for all $i \in \mathcal{F}$ and $q' \in B_x(p)$: $U(i, q_i) \leqq U(i, q^a_i)$. But (3) is identical with (D3-c-4.1-i) and (ii) with $(d^*(i)(p, q^0_i))_{i \in \mathcal{F}}$ instead of q^a. So, since U_i is strictly quasi-concave and monotonic, (D3-c-4.2) yields $q^a_i = d_i(p, q^0_i)$, which is (D1-4.3).

"$\Leftarrow$". Let $x\rho y$ and $y \in M^*_p$. Monotonicity and strict quasi-concavity of $U(i, \cdot)$ follows from (D1-2) and (D3-c-2). Let $q \in E$. By (D3-c-3): $q = q^a$, and by (D1-4.3) and (D3-c-4.4) we obtain (5) $q^a_i = d_i(p, q^0_i) = d^*(i)(p, q^0_i)$. (D3-c-4.1-i) yields (6) $p \cdot d^*(i)(p, q^0_i) \leqq p \cdot q^0_i$. But (5) and (6) together imply $p \cdot q^a_i \leqq p \cdot q^0_i$, and so $q = q^a \in B_x(p)$ which proves (D0-2.1). In order to prove (D0-2.2) let $q \in E$ and $q' \in B_x(p)$. As above we obtain $q_i = d_i(p, q^0_i)$ and $q' \in B_x(p)$ yields $p \cdot q'_i \leqq p \cdot q^0_i$ for $i \in F$. By (D1-4.2) this implies $U_i(q'_i) \leqq U_i(d_i(p, q^0_i))$, and from (D3-c-2) we obtain $U(i, q'_i) \leqq U(i, d_i(p, q_i)) = U(i, q_i)$. (D0-2.3) follows from (D3-c-3).

(b) Let $y \in M^*_p$. Define x by (D3-c-1), (2) and (3) and let $d^*(i) = d_i$ for

$i \in F$. Then (D1-4.2) implies (D3-c-4.1). The other parts of (D3-c) are satisfied because of the definition of x. So $x\rho y$, and, obviously, x is uniquely determined by y. So $\check{\rho}$ is a function. Also, if $y_1 \neq y_2$ and $x_1\rho y_1$, $x_2\rho y_2$ then (D3-c-1), (2), (3) will yield a difference of x_1 from x_2. That is, $\check{\rho}$ is injective. Finally, let $x \in M^c$. Define F, G′, $(q_i^0)_{i\in F}$, $(q_i^a)_{i\in F}$ and p' by (D3-c-1), (2), (3). For $p^* \in \mathbf{R}^m_{++}$, in M^c there is some $q(p^*)$ such that (D3-b-3.1) and (2) are satisfied. Define, for $i \in F$ $d^*(i): \mathbf{R}^m_{++} \times \mathbf{R}^m_+ \to \mathbf{R}^m_+$ as follows. For $p^* \in \mathbf{R}^m_{++}$ let $d^*(i)(p^*, q_i^0) = q(p^*)$, and $d^*(i)(p^*, q_i)$ be defined arbitrarily, if $q_i \neq q_i^0$. Then (D3-b-3) implies (D4-c-4.1). If we set $d_i(p^*; q_i) = d^*(i)(p^*, q_i)$ then (D4-c-4.2) is satisfied, too. That is, for $y = \langle F, \ldots, (d_i)_{i\in F}\rangle$ we have $x\rho y$, and $y \in N$ holds by construction of y. Hence $\check{\rho}$ is surjective.

NOTES

[1] Fellow at the Netherlands Institute for Advanced Studies (NIAS) 1982/83. I am indebted to B. Hamminga and E. W. Händler for helpful remarks on an earlier draft.

[2] For later reference it may be convenient to repeat the axioms in brief.

D0-1. $x = \langle \mathcal{F}, G, q^0, p, U, E\rangle \in M_p$ iff

(1) $\mathcal{F}$ is a finite, non-empty set and $G = \{1, \ldots, m\} \subseteq \mathbb{N}$;

(2) $q^0: \mathcal{F} \times G \to \mathbf{R}_+$, $p: G \to \mathbf{R}_{++}$, $U: \mathcal{F} \times \mathbf{R}^m \to \mathbf{R}$ is smooth and $E \subseteq \{q/q: \mathcal{F} \times G \to \mathbf{R}_+\}$.

Let $B_x(p) = \{q: \mathcal{F} \times G \to \mathbf{R}_+ / \forall i \in \mathcal{F}\ (\sum_{g\in G} p(g)[q(i, g) - q^0(i, g)] = 0)\}$.

D0-2. $x \in M$ iff $x \in M_p$ and

(1) $E \subseteq B_x(p)$,

(2) $\forall q(q \in E \to \forall i \in \mathcal{F} \forall q' \in B_x(p)(U(i, q'(i, 1), \ldots, q'(i, m)) \leqq U(i, q(i, 1), \ldots, q(i, m))))$

(3) $E \neq \emptyset$.

Elements of M_p are called potential models, elements of M models. $B_x(p)$ is the budget set of x. I adopt Haslinger's notation so that $\mathbf{R}_+ (= \mathbf{R}_0^+)$ and $\mathbf{R}_{++} (= \mathbf{R}^+)$ denote the sets of non-negative and positive real numbers, respectively. (D0-2) is misprinted in [B]. For further explanations see [B]. The concepts $\bar{q}$ and Z_x from [B] have been omitted here for reasons of simplicity, and also because they seem to be not really necessary – at least not in the present formalism. Also, I would not call the elements of E "equilibrium" distributions any longer, for reasons to become clear in section II.

[3] See [H], Def. (2), p. 126. Haslinger uses F and G as if they were finite segments of $\mathbb{N}$ though he only requires them to be sets. For the sake of an easier comparison we adjust his formulation to ours in this point. Our (D1-4.2) is just a more explicit version of his (Def. 2-7).

[4] This has to be taken *cum grano salis* because of the problems of reconstructing theories including classical analysis (as PEE) in the frame of first-order logic.
[5] Note that in the proof of (T3) the dependence of f (which corresponds to our d) on q^0 is not made explicit. However, the proof works for each given q^0 thus producing, in effect, demand as a function of prices and q^0.
[6] The distinction between basic laws and special laws must not be confused with the distinction between a "theory" and "auxiliary hypotheses".
[7] I confess to be guilty of not having been strict in the terminological distinction between "theory" (as theory-net) and "theory-element" in [B]. Often, I used "theory" where "theory-element" would have been appropriate the reason being that a theory-net of exchange theory was treated only as a minor item. For a definition of "theory-net" compare [Balzer and Sneed, 1977/78].
[8] Dom(R) denotes the domain of R. Since R in the present case is a real-valued function, there exists some D such that $R: D \rightarrow \mathbb{R}$. Dom(R) then just is D.
[9] In the usual (first-order) model-theoretic sense. See e.g., [Shoenfield, 1967], p. 81. The present considerations are mere heuristics because, as already mentioned, there are problems of reconstructing empirical theories (which usually, like PEE, contain classical analysis) in first-order logic. Compare [Balzer, 1985] for a discussion.
[10] (D2) modifies Gaehde's original account in two major respects. First, I consider theoreticity as a property of single terms (and not of collections of terms). Second, I can avoid an explicit relativization to a *given* invariance of the theory.
[11] See [Balzer, 1984a] for a detailed formal proof. In this paper the new definition also is applied to classical particle mechanics and to classical collision mechanics.
[12] I am indebted to U. Gaehde and W. Stegmüller for illuminating discussion of this topic.
[13] This problem is discussed in more detail in [Balzer, 1983] where I speak about a "logical problem of confirmation".
[14] At least as long as no "Einsteinian revolution" takes place in economics.
[15] Compare [Balzer and Sneed, 1977/78] for structuralist notions of equivalence.

REFERENCES

Balzer, W.: 1982, 'A Logical Reconstruction of Pure Exchange Economics', *Erkenntnis* **17**, 23–46.

Balzer, W.: 1983, 'Theory and Measurement', *Erkenntnis* **19**, 3–25.

Balzer, W.: 1984, 'On a New Definition of Theoreticity', to appear in *Dialectica*.

Balzer, W.: 1985, 'Incommensurability, Reduction, and Translation', to appear in *Erkenntnis*.

Balzer W. and J. D. Sneed: 1977/78, 'Generalized Net Structures of Empirical Theories', *Studia Logica* **36**, 195–211 and **37**, 167–194.

Gaehde, U.: 1983, *T-Theoretizität und Holismus*, Frankfurt/Main-Bern.

Haslinger, F.: 1983, '"A Logical Reconstruction of Pure Exchange Economics": An Alternative View', *Erkenntnis* **20**, 115–129.

Shoenfield, J. R.: 1967, *Mathematical Logic*, Reading, Mass.

Sneed, J. D.: 1979, *The Logical Structure of Mathematical Physics*, Dordrecht, second edition.
Stegmüller, W.: 1979, *The Structuralist View of Theories*, Berlin-Heidelberg-New York.

Manuscript received 25 February 1984

Institut für Statistik und Wissenschaftstheorie
Universität München
Ludwigstr. 31
8000 München 22
F.R.G.